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On certain Suspected Changes in the Lunar Surface.

By the Rev. T. W. Webb.

(*Communicated 13th May*, 1864.)

It is now precisely five years (even to the day), since I had the honour of submitting to the Royal Astronomical Society a notice of traces of eruptive action in the Moon, in which I drew attention to the spots *Cichus* and *Messier*, as furnishing probable evidence of the continuance of explosive force during the present century. I had hoped, in the intervening time, to have entered somewhat further into this curious subject; but various hindrances, among which may be especially mentioned the prevalence of cloudy skies, have combined to interfere so materially with my purpose that, even at this distant period, I have but little to communicate. Small, however, as my contribution to Selenography must appear, I beg permission to offer it to the Society, under the impression that, in a work of such extent as the delineation of the Lunar Surface, every additional fact, even if insignificant in itself, possesses a certain amount of relative value.

Being unable to adduce any evidence of change from my own observations, and, as it happens, not having hitherto particularly studied the portions represented in the Sections of Lohrmann, I have uniformly referred, for the purpose of comparison, to the great Map of Beer and Mädler. In doing this, I have been obliged to notice, with concern, occasional indications of a want of that high accuracy which might have been expected from the general character of the work. I have never entertained a doubt as to the correctness of their triangulation, or of the laying down of such primary points as are immediately connected with it; but it has appeared to me that an equal amount of dependence cannot always be placed upon the subsequent filling in, by mere eye-estimate, of lesser details. This is much to be regretted, not only as diminishing in a general sense the confidence with which every observer would wish to regard this truly great undertaking, but as disappointing us more especially in that very respect in which a reliable standard of comparison would have been so desirable. It seems generally admitted that no alteration of any considerable magnitude has taken place upon the lunar surface since

the date of anything which can be called accurate observation; it is only in the smallest class of craters, therefore, that we can reasonably look for traces of continued activity; and it is precisely *there* that we become sensible of a deficiency in the work of the illustrious German astronomers; in some instances we find these minute details laid down with less regard to accuracy of position than would have been desirable; in others, we are inclined to question whether they actually represented all that their instrument was capable of showing them, and that they, therefore, might have seen, especially if they had been willing to avail themselves of the labours of their predecessors. It is, in fact, difficult to reconcile the extraordinary pains and diligence which characterise the larger part of this grand undertaking, with the incorrectness which mars some of its details; it is very difficult to understand how it should have been done so well, and yet not have been done better. It would be easy to give proof of what has now been asserted, especially in reference to a comparison with older delineations: we have one remarkable instance in the great plain recently termed by Dr. Lee the *Mare Smythii*, where Schröter's figure, under the name of Abraham Gotthelf Kästner, is much more faithful than their own; and another, in the *Sinus Aestuum*, where their assertion that no craters are discernible was afterwards retracted by Mädler, and never, one would suppose, could have been made had they consulted the design of Lohrmann. In these and similar cases, the accuracy of their map seems to have suffered from their unwillingness to be under obligation to those who, if in some respects inferior to themselves, had a right, as credible witnesses, to a fair share of attention. From the persevering investigations of Mr. Birt it would appear that their deficiencies are very obvious in the regions adjacent to the limb, where, it may be admitted, the difficulty of delineation was greatest: my own observations, referring to more central regions, where errors of such magnitude would naturally not occur, still indicate, from time to time, want of care in the representation of minute features. And, though allowance must be made for the superiority of my instrument, this cannot be extended very far, as the extreme minuteness and perfection of their detail in many cases show what might have been attained in all. It is the unfortunate want of confidence in our highest standard thus necessarily resulting which throws some doubt upon the evidence of change which I am about to adduce, and renders an appeal to the future still necessary to render it conclusive.

Of the various observations in my journal, some relate to objects so minute that, though I have carefully recorded them, on account of their possible prospective value, they find no place in the present inquiry; others are not satisfactory to myself, or require confirmation: a few only have been selected, which seem the best established, and which I feel the least hesitation in recommending to the attention of other observers.

I. *Helicon* A, according to Beer and Mädler's nomenclature, or *Leverrier*, as it has been designated by the French astronomers.

As far back as 1836, May 26, I had noticed, with an achromatic telescope of $3\frac{7}{10}$ inches aperture, and a power of 144, a crater which I then described as "extremely minute, about the *minimum visibile* with the present weather and magnifying power," lying close to the ring of *Leverrier* on its S.E. side.

1861, April 20, after I had completely forgotten the previous observation, I found the same object, "very conspicuous," owing, perhaps, to the greater power of my present telescope; and since that time I have repeatedly seen it with perfect ease.

II. *Helicon.* At the date of my rediscovery of the last object, "I was at once struck," as I have expressed it in my journal, "by the unusual appearance of a minute, but unquestionable, crater on the summit of the E. wall of *Helicon;*" and I have, on many subsequent occasions, found it very visible; and even been able once to make out the shadow in its interior; it is entered as "most obvious" when the terminator was passing through *Aristarchus;* and in one evening it was seen even when that line was so remote as to include *Schickhard.*

III. Crater in the *Mare Imbrium.* In the direction from *Leverrier* towards *Archimedes*, a little N. of the letter b in the word "Imbrium" in Beer and Mädler's Map, is a small crater, which they have not inserted, notwithstanding its distinctness and insulated situation in the midst of the open plain. A little marking, which catches the eye on the map near its place, will be found, on closer examination, to be nothing more than a defect in the engraving. I first noticed the crater, 1861, May 18, when it was more conspicuous than several small craters in the neighbourhood figured by Beer and Mädler; and I have since seen it on other occasions without difficulty.

IV. *Kirch.* Between this object and a similar crater to the N.W., marked A in the map, and referred, as it appears, to *Pico* as head-quarters (why, it is not evident), I found, 1861, Dec. 10, a minute but very obvious crater of an elliptical form, containing a distinct shadow. This is only one of several omissions in the same neighbourhood, but the rest, like other minute craters in the *Mare Imbrium*, are so small that they cannot be fairly adduced in evidence.

V. *Delisle* b, a crater of moderate dimensions in a line between *Carlini* and *Wollaston*, has a small crater on the N. end of the ring, not to be found in the map, notwithstanding its obvious aspect and conspicuous situation. I first noticed it, 1863, September 23.

VI. *Diophantus.* Just S.E. of this object lies a very visible little crater, omitted by Beer and Mädler, but observed by myself, 1863, September 24.

VII. *Copernicus.* 1862, Feb. 8, I found the S. and S.W. slope of this magnificent ring thickly studded with very minute

craters, not represented by Beer and Mädler. The omission is chiefly remarkable, as they form a continuation of the extraordinary assemblage of similar foci of eruption lying between *Copernicus* and *Eratosthenes*. This latter wonderful district, it may be observed by the way, has probably assumed its present honeycombed aspect during the present century, as it is hardly conceivable that in such a situation it should have escaped the persevering scrutiny of Schröter, and been left for the eye of Gruithuisen in 1815.

VIII. *Mersenius*. This ring, as is well known, is remarkable for the very unusual feature of a convex interior, which was discovered and well drawn by Schröter, and appears then to have had no object upon its summit. It is represented equally smooth in the Map of Beer and Mädler; and, from their description, we are assured that this was no error in the drawing, for they speak of several inconsiderable ridges and terraces round the inner foot of the ring, and a general upheaving of the interior towards the centre, so that for some hours this bubble-like convexity casts a distinct shadow to the eastward in the increasing Moon, and they have, though opportunities are not so frequent, seen it falling in the opposite direction; but they expressly add that "a central mountain is totally absent." No further evidence can be needed to show that they had thoroughly investigated the spot. Now, in the year 1836, May 27, I perceived with my smaller achromatic, on the summit of this well-known convexity, a minute crater and several other delicate markings, which I never thought of subsequently till 1855, Oct. 22, when I caught an indistinct sight of them again. 1863, Sept. 24, I had a much better view of them, though the air was not very favourable; they had disappeared on the following morning; and I have no doubt they may at any time be readily verified under suitable illumination with an instrument of moderate power. The aspect of a considerable portion of them is that of a line of long irregular ravines without elevated margins, as though they had been formed by the dropping in of part of an inflated and hollow crust. We have here something that certainly looks like evidence; and it would seem as though the epoch of the supposed convulsion might be fixed within narrow limits; for the Map was published between 1834 and 1836, and the letterpress in 1837, both probably from sketches and memoranda of a somewhat earlier date; while my first detection of these objects took place in 1836. Gruithuisen speaks of a small cleft on the central convexity of *Mersenius* in a work published in that year; but gives no date of observation, merely including it in his general remarks on a number of similar features, with some of which he seems to have been long familiar.*

* I have since found that Lohrman has inserted a minute crater on the convexity in his map, made from observations taken between 1822 and 1836. —T. W. W.

The foregoing instances seem worthy of notice, from their sufficiently, and in several cases peculiarly conspicuous situation, in juxtaposition for the most part with objects well known and frequently observed. Some of them were detected with an instrument inferior to that employed by Beer and Mädler; and I should not have called attention to any of them unless they had been of greater magnitude and visibility than other features which they have faithfully represented. Now, in order to account for these presumed changes, it has appeared to me that only three explanations are possible. However singular may be the variations of aspect due in certain cases to libration, the objects which have been mentioned are so well marked out, and so readily and permanently visible, that their non-appearance cannot in fairness be referred to that cause: a little experience in lunar observation would convince us that they do not belong to a class whose inconstancy of appearance can be explained in that way. It remains that they must be accounted for, as far as I can see, either by the supposition of inadvertency on the part of former selenographers,— of actual physical alteration of surface,— or of some kind of obscuration arising from eruptive action, or from the changed conditions of a possible lunar atmosphere,— an alternative due to Schröter, who frequently had recourse to it, in order to explain appearances, of which it seemed to him that no equally reasonable account could be given. Any other supposition, besides these, I confess that I have been unable to devise. Nor do I presume to offer an opinion as to the comparative probability of those I have mentioned. I would prefer to leave the subject in the hands of those better qualified by superior instrumental means and longer practical experience to form a satisfactory judgment. But, before bringing these remarks to a close, I would beg to direct attention to one more singular instance of variation, which sets in a very strong light the difficulty and uncertainty attendant upon these interesting researches. It will be that of the well-known object —

IX. *Gassendi.* This great walled-plain would naturally, from its position, attract the attention of all selenographers; it was twice delineated by Schröter in different circumstances of libration and illumination; and in the Map of Beer and Mädler it has all the appearance of careful drawing, while the corresponding description is unusually full and precise. They speak of an interior containing in the Full Moon two rows of luminous specks, whose junction is marked by a bright spot, which they measured for position on nine several occasions. They then tell us how, as the illumination declines, this spot disappears, together with the majority of the other specks, and a central mountain only four or five miles S. of it comes into view; while, when the angle of illumination is very small, the same spots return again as mountains, not in the previously described form, but as longish ridges stretching in various directions

among a great number of smaller irregularities in every part of the surface, so that any one who had not watched *Gassendi* attentively from night to night would never recognise, in a certain little inconspicuous hillock, the glittering point prominent in the Full Moon, but would rather be inclined to suppose some physical change: and yet, they add, all this ensues, as we have described it, in every lunation. With all this minute recital before him, an observer, who, like myself, found, on the evenings of Sept. 23, 24, and 25, in last year, a minute, but brilliant, crater standing out so clearly and sharply upon the floor, that there could be no possible doubt of its nature; and on Oct. 23, a second of the same dimensions near it, perfectly decided and unquestionable, might well have been excused for supposing that actual outbursts must have occurred since 1837 in this thoroughly explored region. What, then, would be his surprise to find that he was quite wrong in this idea, and that these two small craters had both been seen and drawn as far back as 1797, by Schröter, who himself expresses his wonder that he had never noticed them before, even in using a larger instrument, during the observations of nearly four years?

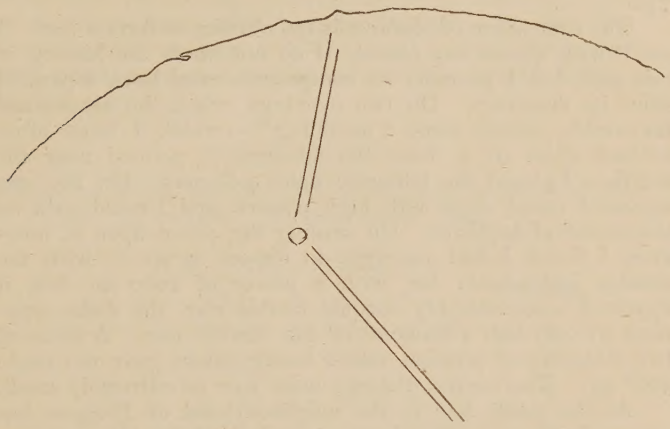
I will not occupy the time of the Meeting by a description of other singular and perplexing appearances which I have met with in the interior of this walled-plain, especially as the express investigation of it is known to be in better hands. Nor is there any occasion for a lengthened comment upon this curious instance of alternate visibility and invisibility. Insignificant as it is in point of magnitude, it will be seen at once to be eminently suggestive of the necessity of caution in drawing inferences from appearances with which we have not been long and thoroughly familiar. We would not press it too far against the probable evidence of changes in our own time, when we are well assured that they were frequently taking place at some former time, and can assign no sufficient reason why they should not be still in progress; but it may teach us the danger of forming precipitate conclusions; and we may add that the observer who will succeed in giving a satisfactory explanation of these discrepancies will have made no small advance in clearing up the difficulties of selenography.

Account of his Observations at Malta. By W. Lassell, Esq.

(*Extract from a Letter to the Rev. C. Pritchard.*)

On the 21st instant, about 8 P.M., G.M.T., the atmosphere not being fine enough for advantageous use of the large telescope, I went into the small dome, in which I have recently erected the 9-inch Equatoreal, originally mounted at Starfield,

many years ago. The large speculum, however (9·1 inches diameter and 112 inches focus), now in the tube, was refigured only about five years since. On turning this instrument on the Moon, I believe with no more precise object than to learn the state of the atmosphere with this aperture — an eye-piece magnifying 199 times having been applied—I was immediately struck with an unusually irregular outline of the Moon's limb, and was reminded of a rather startling diagram I had seen in a paper by the Rev. Henry Cooper Key, in the *Monthly Notices*, vol. xxiv. page 20. I made a drawing at once in my journal, of which this is a fac-simile:—



A careful comparison afterwards of the diagram with the Moon convinced me that it might be considered optically correct. The position of *Tycho* and two of the principal lines radiating from it, given in the diagram, will point out pretty nearly in what part of the Moon's limb these irregularities were observed. But it will be seen that in proportional magnitude they are *very much* less than those described by Mr. Key, the length of the flat portion — (I am not sure that it was really *concave*)—not extending over more than about 11 degrees of latitude. As the definition of this telescope always excites my admiration, and is, indeed, not surpassed by that of any telescope of any form that I have seen, I think that no "irradiation" could have a sensible effect "in obliterating the true outline" of the Moon's limb. It appears, however, that very different parts of the limb were observed in the two instances.

While speaking of this telescope, may I be pardoned if I say a word or two as to its quality; for I have not been so long familiar with large telescopes as to make me insensible to the merits of small ones.

The "sixth star" of the trapezium of the nebula in *Orion* is easily seen with it by an ordinary eye with powers from 200

to 300. I saw it best on the 3d March last with a single lens magnifying 291 times. *Enceladus* is well seen when near its greatest elongation, in the present position of *Saturn's* ring,—about as easily as *Mimas* is in the 4-foot Equatoreal; unless the air be very tranquil, when the latter has the advantage. The small star near δ *Cygni*—that severe test for all telescopes—exhibits a palpable disk, not to be overlooked by a casual observer, even in a barely average state of atmosphere. I have seen it best when the air was quiet, with a power of 471. The very minute and faint double star μ^2 *Boötis* it now separates by the finest line, under favourable circumstances, with a power of 732.

The companion of *Antares* is too obvious to form a test. I see it with almost any power. I do not know the history of this star, but I presume its components must have separated since its discovery. On two evenings, when the air seemed favourable, except some "moulding"—which I have often noticed when in a dome the telescope is pointed near the zenith—I placed the telescope upon γ *Coronæ;* but the star appeared round even with high powers, and I could gain no impression of duplicity. On turning the 4-foot upon it, however, I found I had no right to expect to see it with the smaller instrument; for, with a power of 1060 on this, it appeared a considerably unequal double star, the disks separated by only half a diameter of the smaller one. A mean of two measures of position, rather hastily taken, gave the angle 306° 45′. The central distance must now be extremely small.

As the small star in the neighbourhood of *Procyon* has attracted some attention lately, I send the following measures of it in position and distance, made by Mr. Marth and myself, and would have sent them earlier if they had tended to establish the hypothesis of any physical connexion with the large star:—

1864·15	position	295°·72	Mean of 6 measures,		greatest diff.	2°·73
	distance	44″·95	,, 3	,,	,,	0″·29
1864·20	position	296°·97	,, 6	,,	,,	2°·00
	distance	44″·30	,, 6	,,	,,	0″·94
	position (L.)	295°·19	,, 5	,,	,,	1°·47

I come now to the chief object of my letter, which is to hand you the results of our observations of *Uranus* and *Neptune*, which, remembering what I was able to do in the splendid, and, as it now appears, exceptional season of 1852, are much less numerous than I expected. It must be remembered, however, that a much finer sky is demanded by a 4-foot than a 2-foot aperture. Except where the letter L is appended, the observations have been made by Mr. Marth, who continues to add to our list of new nebulæ.

I have made a good many drawings of planetary and other nebulæ, but have been so much disappointed in some attempts I made to have them faithfully represented in England, that I hesitate about their publication. I have lately tried an Italian artist, whom I have met with here, and I inclose a specimen of his work, which pleases me somewhat better, though the shading-off of the nebula is somewhat too abrupt, and the proportional magnitudes of the stars are not perfectly preserved.* It is the large nebula, *Messier* 20, R.A. $17^h\,54^m{\cdot}2$, N.P.D. $113°\,2'$ (1865), and is figured in Sir John Herschel's *Cape Observations*.

We are at present much occupied with *Saturn* in observing, amongst other phenomena, conjunctions of the satellites (especially the closer ones) with the poles of *Saturn*, or with tangents to the limbs or ansæ of the ring, with a view to contribute towards a more perfect knowledge of the forms of their orbits.

Malta, May 28, 1864.

Observations of the Satellite of Neptune, and of the Satellites of Uranus. By W. Lassell, Esq.

The Satellite of Neptune.

1865.	G.M.T. h m			Measures.
Sept. 10	12 0·9	pos.	215°·69	5
	12 39·3	dist.	14″·15	6
	13 6·4	pos.	213°·96	5
11	11 48·0		212·15	10
	12 35·4	dist.	15″·95	2
14	12 42·0	pos.	30°·34	4
17	11 34·5		210·55	6
19	11 37·1		33·50	6
	12 35·6	dist.	15″·52	3
23	12 27·0	pos.	211°·69	8
28	11 18·2		215·12	6
Oct. 5	10 6·8		212·68	4 L.
	10 8·9		211·73	4
6	11 1·0		39·3	2
10	10 1·0		213·80	8
31	8 30·0		34·11	8
	9 14·6	dist.	16″·25	8
Nov. 14	8 11·6	pos.	216°·51	6
Dec. 11	6 41·9		35·14	8
	7 23·9	dist.	16″·92	8

* An engraving from the drawing referred to will be given in the volume of the *Memoirs*.— Ed.

Observations of the Satellites of Uranus.

Oberon.

	G.M.T. h m			Obs.
1863. Dec. 14	10 38·5	pos.	41°·14	2
	11 7·9	dist.	45″·27	2
(L.)	11 17·6		44·85	3
16	10 37·2	pos.	347°·46	2
1864. Jan. 22	7 44·3		81·88	2
	8 13·0		82·24	,,
27	8 5·6		307·44	,,
28	7 43·3		281·05	,,
	7 49·9		282·14	,,
29	9 42·8		254·20	,,
	10 12·4		252·61	4
Feb. 5	6 49·9		66·81	2
6	6 28·7		41·20	4
26	8 17·8		226·79	2
	8 53·4		225·44	,,
27	8 16·5		196·29	,,
	8 50·1		197·31	,,
Mar. 2	7 26·6		93·04	3
3	8 17·5		65·08	2
7	7 15·8		318·83	,,
	8 27·8		317·74	,,
8	8 26·2		291·38	,,
10	6 46·1		240·21	,,
(L.)	7 32·4		239·41	3
(L.)	7 50·0	dist.	43″·14	1
	7 56·8		43·03	3

Titania.

	G.M.T. h m			Obs.
1863. Dec. 14	10 33·4	pos.	71°·11	2
	11 0·0	dist.	33″·76	,,
16	10 38·1	pos.	345°·64	,,
1864. Jan. 22	7 53·1		262·65	,,
	8 4·7		261·80	,,
27	8 4·5		53·18	,,
28	7 35·4		12·40	,,
	7 57·2		12·15	,,
29	9 50·1		327·93	,,
	10 1·8		329·35	,,
Feb. 5	6 43·8		45·25	,,
6	6 29·3		3·42	4
26	8 11·7		255·71	,,
	9 0·5		252·85	,,
27	8 11·5		210·41	,,
	8 57·1		209·84	,,
Mar. 2	7 36·0		49·51	,,
3	8 25·3		3·33	,,
7	7 44·0		200·89	,,
	7 50·0		200·80	,,
8	8 34·4		157·29	2
10	6 53·1		77·25	,,
(L.)	7 24·3	(L.)	77·26	2
	8 9·6	dist.	33″·09	3

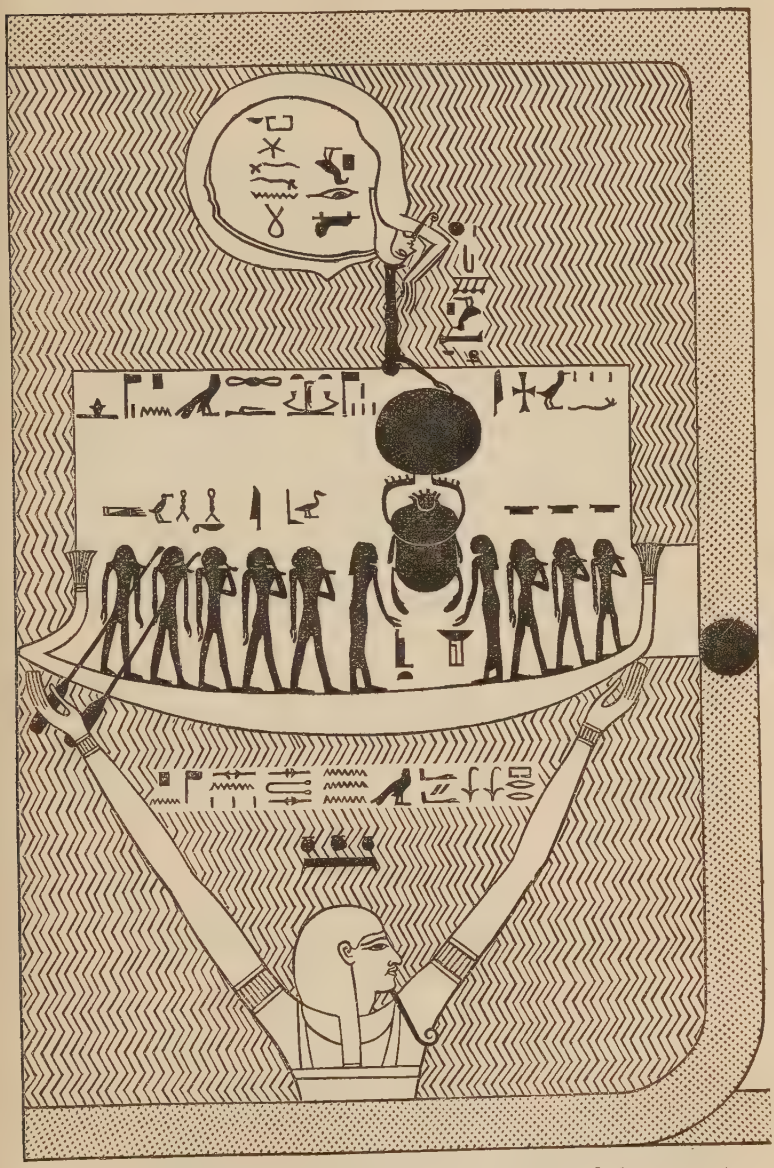
Umbriel.

	G.M.T. h m			Obs.
1863. Dec. 14	10 48·8	pos.	8°·99	2
1864. Feb. 26	8 34·3		72·42	,,
	8 44·8		72·19	,,
27	8 2·9		344·71	,,
Mar. 2	7 53·3		0·86	,,
3	8 33·1		269·61	,,
7	7 35·7		289·30	,,
	8 4·2		287·80	,,
10	7 6·0		27·14	,,

Ariel.

	G.M.T. h m			Obs.
1863. Dec. 14	10 42·3	pos.	86°·79	2
1864. Feb. 26				
27	8 26·1		185·76	,,
Mar. 2	8 6·6		333·30	,,
3				
7	7 25·4		346·78	,,
	8 15·6		343·15	,,
10	6 59·6		281·74	,,

On the Astronomy of the Ancient Egyptians, as shown on the Belzoni Sarcophagus. By Mr. Bonomi.



It may not be uninteresting to Astronomers of the present day to understand from an authentic document what were the

notions entertained by the most learned people of antiquity, 1250 years before our era, respecting the construction of the Universe.

The drawing is faithfully copied from a diagram engraved on the wall of a sarcophagus, which was made to contain the body of a king of Egypt, of the period above mentioned, viz., 1250 years before our era.

In the upper part of the diagram is a figure of Osiris, curled up in the attitude of a tumbler, to represent the Earth floating on the surface of the great ocean, which is here signified by the zigzag lines that surround him. This idea of the Earth floating on a vast expanse of water was entertained by all the nations of antiquity, and there is allusion to it in the 24th Psalm, wherein it is stated that the Almighty "founded it upon the seas, and established it upon the floods:" and again, in Psalm 136, verse 6, "To him that stretched out the earth above the waters." Likewise, in the Apocryphal writings, as, for instance, in the 2d book of Esdras, chapter 16, verse 58, it is said, "He hangeth the earth upon the waters."

So likewise in the first century of our era, in the second epistle of Peter, it is said, "By the word of God the heavens were of old, and the earth standing out of the water and in the water;" showing that, although the globular figure of the Earth might have been satisfactorily demonstrated by Demosthenes of Cyrene to a few learned men in Alexandria 200 years before, it was still the popular notion at the time Peter wrote his second epistle.

The little figure standing on the head of Osiris is the goddess of heaven, Netpè, supporting the Sun, which is connected by the Scarabæus with certain divinities in a boat, held in the outstretched arms of a figure, supposed to represent the Ocean. This part of the picture relates to some Egyptian dogma respecting the soul, which has not yet received a satisfactory explanation.

List of Radiant Points of Shooting-Stars.

By Professor Heis.

(*Extract of a Letter from Alexander S. Herschel to Mr. Pritchard.*)

Mr. Williams having kindly sent to the Astronomer Royal at Münster (Dr. Heis), at my request, the bundle of star-charts filled-in with shooting-stars by Mr. Greg, I to-day received a reply from that gentleman, containing a list of Radiant Points observed in eleven years before 1860, at other times of the year than in August and November. Dr. Heis purposes to publish the account of these observations at length

in the ensuing winter. The list I now enclose, with the remark that δ *Virginis* is there pointed out as a perspective vanishing point of shooting-stars, observable *in the months of April and May*. This position I detected by independent observation, *in a single evening*, on the 10th of April last. On the morning of the 20th, following, meteors were also watched for, and were again plentiful. The perspective vanishing point of their courses was fixed with all the accuracy desirable at *Wega Lyræ*.

α *Lyræ*, like the foregoing star δ *Virginis*, is an acknowledged Radiant Point, having been observed by Herrick, in America, on the 19th April, 1839 (*American Journal of Science*, 1st series, vol. xxxvi. p. 362). The observations on the 10th and 20th April, 1864, establish, therefore, two important points:—

1. The interval of a quarter of a century may produce *no sensible alteration in the direction of a meteoric stream.*

2. Two meteoric streams may arrive at the Earth at an interval of not more than ten days apart, *almost exactly transverse to one another in direction.*

These facts are exceedingly difficult to explain upon general principles: both are, however, perfectly conformable to the hypothesis that shooting-stars are cosmical bodies revolving round the Sun.

Radiant Points of Shooting-Stars, Jan.—Dec.

			α = R.A.	δ = N. Decl.
Jan.	1—15	K_1	242°	+51°
		A_1	29	+50
		M_1	135	+57
		N_1	285	+84
Jan.	16—31	K_2	235	+62
		A_2	15	+63
		M_2	166	+52
		N_2	N. Pole	
Feb.	1—14	A_3	65	+51
		M_3	150	+60
		N_3	N. Pole	
Feb.	15—28	A_4	91	+37
		M_4	130	+63
		N_4	250	+83
		S_1	170	+11

		α = R.A.	δ = N. Decl.
		°	°
March 1—15	A_5	54	+47
	M_5	125	+52
	N_5	340	+80
	S_2	178	+ 7
March 16—31	A_6	35	+45
	M_6	140	+50
	S_3	173	+ 23
	N_6	N. Pole	
April 1—15	A_7	80	+45
	M_7	160	+53
	S_4	181	+18
	N_7	260	+86
April 16—30	A_8	10	+54
	M_8	150	+61
	S_5*	194	+ 5
	N_8	265	+83
May 1—31	S_6*	198	+ 4
	N_9	290	+80
	Q_1	218	+20
	B_1	332	+60
June 1—30	B_2	333	+42
	Q_2	242	+12
	N_{10}	750	+83
	W	292	+15
July 1—15	A_9	41	+62
	B_3	315	+54
	Q_3	262	+12
	N_{11}	20	+85
Aug. 15—31	A_{10}	35	+61
	B_4	306	+59
	N_{12}	295	+79
	T_1	314	+15
Sept. 1—15	A_{11}	35	+63
	B_5	293	+57
	N_{13}	130	+84
	T_2	343	+10
	R_1	53	+35

		α = R.A.	δ = N. Decl.
		°	°
Sept. 16—30	A_{12}	44	+63
	B_6	311	+65
	N_{14}	65	+84
	T_3	1	+11
	R_2	46	+37
Oct. 1—15	A_{14}	51	+61
	B_7	315	+65
	N_{15}	105	+88
	R_3	45	+32
Nov. 19—30	A, N, C, L	Agreeing nearly with the points determined for Nov. 10—13.	
Dec. 15—31	A_{16}	37	+59
	N	340	+34
	M_9	137	+59
	K_3	235	+52

Collingwood, Hawkhurst,
May 10, 1864.

* The Right Ascension and Declination of δ *Virginis* are the following, viz., $\alpha = 192°$, $\delta = +4°$.

On a Zenith Sector for the use of the Great Trigonometrical Survey of India. By Lieut.-Col. A. Strange.

In former times the Zenith Sector employed on the British Ordnance Survey and on the Great Indian Survey consisted essentially of a long telescope, suspended by its upper or object-glass end, and traversing over a short arc at its lower or eye-end, the radius of the arc being equal to the focal length of the telescope. In all cases reference was made to gravity by means of the plumb-line. This form of instrument involved some uncertainty as to the centre of the circle of which the short arc or sector formed a part; and the flexure of the telescope and the effects of expansion on the angular value of the arc were anxious considerations; moreover, the plumb-line is an adjunct difficult to use, and inferior in point of delicacy to the spirit-level; further, observations to the same star could not be taken in reversed positions of the instrument on the same night: and, finally, the instrument required for its

support a lofty and cumbrous fabric of wood, very inconvenient to move and set up.

This form of Sector by Graham and Ramsden is well represented in Pearson's *Practical Astronomy*. That by Ramsden is remarkable for the ingenuity with which it abounds. This fine instrument was unfortunately destroyed by fire in the Tower of London.

A new instrument was therefore designed for the Ordnance Survey by the present Astronomer Royal. It is fully described in the Ordnance Survey Reports, from which I extract the following passage:—

"The first principle in this Instrument, now known as Airy's Zenith Sector, was the arrangement for making successive observations in two positions of the instrument—face East and West at the same transit. The second principle was the substitution of a level or system of levels for the usual plumb-line. The third principle was the casting in one piece, as far as practicable, of each of the different parts of the instrument, in order to avoid the great number of screws and fastenings with which most instruments are hampered, and to secure, if possible, perfect rigidity."

As to the soundness and the progressive character of these principles, no competent judges can differ. The instrument itself is one of the most original ever devised, and the work executed with it is of a very high order.

When entrusted by Government with the superintendence of two Zenith Sectors for the Indian Survey, it might have sufficed that I should simply adopt the plan of the Astronomer Royal; but this was prevented by a condition imposed upon me to which he had not been subject, namely, that of extreme portability combined with equal power.

Mr. Airy's instrument weighs altogether 1140 lbs.—a bulk of metal which constituted it, in the words of Colonel Walker, the head of the Indian Survey, "an instrument evidently unsuited for crossing the higher Himalayan ranges."

An examination of the drawings of the instrument, to which I must refer the reader, will show that its weight is chiefly due to the manner in which the azimuthal motion is obtained, namely, round two points external to the telescope and sector. The abandonment of this primary feature involved the abandonment of the whole design; and the subject had to be taken up *de novo*—Mr. Airy's fundamental principles, above cited, being however preserved as a basis.

I hope on some future occasion to furnish a detailed description of the form at which I have arrived, and shall now merely indicate its general characteristics.

Place a German Equatoreal as it would be at the Pole—that is, with its polar axis vertical. Attach to the telescope, but at right angles to it, a frame carrying at either extremity about 40° of a circle. Attach to the declination-socket four

microscopes opposite the above-mentioned arcs. Then the polar axis of the Equatoreal becomes the vertical axis of the Zenith Sector, and the declination-axis the horizontal or transit-axis. The telescope being pointed to the zenith, the sector-frame will be horizontal. The microscopes are arranged in the same way as those of the Greenwich Transit-circle, and have the same description of illumination.

In this arrangement there are certain features that appear to me to be advantageous:—

1st. The vertical axis is screened from sudden changes of temperature within a hollow cast-iron column.

2d. The sector does nothing but bear its own weight.

3d. Its two arcs, being at the extremities of a horizontal diameter, are both under the same conditions with respect to flexure and exposure to temperature.

4th. The telescope turns on a horizontal axis, is of considerable length, and rests on invariable bearings. It and the attached sector will therefore describe a true plane—a condition necessary to the constancy of the microscopic runs, and to the performance of the instrument as a transit-instrument.

5th. The Greenwich or axial system of microscopic illumination, here adopted, is the best at present known.

6th. The eye-pieces of the microscopes, being all four nearly at the same elevation, and that a low one, are easily accessible to the observer without climbing.

There remains only the weight of the instrument to be considered. The work, executed by Messrs. Troughton and Simms, is now in a sufficiently advanced state to enable me to make a close approximation to what it will be. The whole will weigh not more than 600 lbs., or very little more than half the weight of the Ordnance Survey instrument. The weight of the heaviest piece will be 110 lbs., as against 254 lbs., the weight of the heaviest piece of the latter instrument. But I should add that, in the instrument under description, the sector is 4 inches less in radius than in Mr. Airy's instrument; the telescope, however, being the same. I do not believe this diminution of the sector will affect the power or accuracy of the instrument. The dimensions in my instrument are,—focal length of telescope, 48 inches; diameter of circle of which sector is part, 36 inches; or ratio of sector to telescope = 1 to 1·33. In the Greenwich Transit-circle the ratio is 1 to 1·95; the new Paris Transit-circle is about the same; Ertel's and Repsold's Pulkova Circles have ratios respectively of 1 to 1·62 and 1 to 1·75. The proportions I have adopted, as compared with these important modern works, give a preponderance to the circular over the optical member of the instrument.

London, 1864, *June* 10.

On Dr. Steinheil's Stellar Spectroscope.

By W. G. Lettsom, Esq.

At page 105 of Vol. XXIII. of the *Monthly Notices*, in the abstract by Mr. Drach of Professor Donati's *Memorie Astronomiche*, the author speaks of stellar light as never being strong enough to enable one to distinguish its colours; and he states further that, saving a reddish tinge at the minimum-refraction end, the spectra appear of a lavender tint in their whole length.

This may be the case with Professor Donati's apparatus; but with the stellar spectroscope of Dr. Steinheil, of Munich, the colours of the stellar spectra are as distinctly pronounced as when one examines the light of the Sun by means of a prism.

In the spectra of the different stars there is, it is true, a remarkable difference in the colours shown; yet the variety of hues in by far the greater number of those spectra is admirably exhibited.

I find the spectra of the larger stars to be displayed with such brightness, when using the spectroscope in question with my 9-inch refractor, that I can hardly doubt it would give very satisfactory results with stars up to the second magnitude, when applied to instruments of not more than four inches aperture.

The cost of this apparatus, which may be adapted to any instrument without difficulty, is 66 Bavarian florins, or about 5*l.* 12*s.*

Monte Video, 24th March, 1864.

On the Method of determining Heights in the Trigonometrical Survey of India. By Lieut.-Colonel J. T. Walker, R.E. (Abstract.)*

From the origin of the Survey until the year 1856 the heights of all the principal Stations of the Triangulation were invariably determined by the method of reciprocal vertical angles. But the difficulties which beset observations of vertical angles in the plains of India are so great, and the inaccuracies to which they are liable so serious, that these observations were a cause of much anxiety and uncertainty, because, if erroneous, the lengths of the base-lines and all operations emanating therefrom would be erroneous. It was consequently deter-

* This paper has been ordered to be printed *in extenso* in the volume of *Memoirs*.—Ed.

mined to undertake a series of spirit-levellings to fix the heights of the base-lines, and verify a certain number of principal stations, more particularly those situated in the plains previously referred to. The main lines of levels were executed by at least two persons, each furnished with a standard level and pair of staves, one following the other, station by station, at a few minutes' interval. The various sources of error, and the precautions taken in order as far as possible to remove them, are detailed and discussed. It is stated that a comparison between the results of the spirit-levelling operations and those obtained from the Principal Triangulation is found to be highly satisfactory in all instances where the vertical angles were measured at the time of minimum refraction, even where the triangulations had been carried for long distances over the plains already noticed. The average difference between the results obtained at the ends of four circuits, averaging 550 miles in length, is 3·06 feet, and the maximum difference at any station is 8·7 feet, which occurs at a station in the valley of the Indus, 200 miles from the nearest hill station.

In combining the trigonometrical with the levelled values, the latter are assumed to be correct and final; and stations obtained by the former process alone are referred to the nearest station of the lines of levels. The Survey Department has published a volume of Tables of Heights in Scinde, the Punjab, North-West Provinces, and Central India; and the paper was chiefly compiled from the Preface to that volume.

On the Star β Leonis. By G. Knott, Esq.

In his invaluable *Cycle of Celestial Objects*, Admiral Smyth describes this star in the following terms:—"Position 114°·0 (w. 2); distance 298″·0 (w. 1); epoch 1833·47. A standard Greenwich star with a distant companion, on the switch of *Leo's* tail. A $2\frac{1}{2}$, bluish; B 8, dull red; preceded by a 7th-magnitude star in the *n. p.*" And I am informed by the Rev. W. R. Dawes, who kindly called my attention to this object, that in Admiral Smyth's original diagram three stars are figured, the nearest (the B of the above extract) in the *s. f.* quadrant, and the other two at greater distances in the *n. p.* and *s. p.* quadrants respectively.

The present appearance of the field differs in several respects both from the description and the diagram; and I find that in his "*Second Series of Observations of Double Stars*," printed in the last volume of the Society's *Memoirs*, Mr. Powell of Madras suggests that there is an error of quadrant in the *Cycle*, that the angle of position of B should be increased by 90°,

and that the 7th-magnitude star should be described as in the *s. p.*, instead of in the *n. p.* quadrant. This emendation, though not supported by the *diagram*, would bring the *description* into close accordance with the main features of the field as seen in the telescope; but, if this should prove to be the true solution of the difficulty, I am desirous of calling attention to the curious fact that there happened to be a minute star very near to the place of the B of the *Cycle*, the co-ordinates of which, from a series of wire-micrometer measures with my $7\frac{1}{3}$-inch Equatoreal on four nights are, P = 115°·39, D = 303″·6, epoch 1864·38. Its magnitude I estimate at about 11·7 of Mr. Pogson's standard scale (corresponding to about the $13\frac{1}{2}$ mag. of Admiral Smyth's scale); and Mr. Baxendell informs me that he had himself observed the star some years ago, and estimated it to be of about the 12th magnitude. For the two stars mentioned by Mr. Powell, and which are Nos. 160 and 162, Hora XI. of Piazzi's Catalogue, I obtained the following results: for the former, which is of the 6·5 mag., P = 201°·6, D = 18″·9 ± ; and for the latter, the magnitude of which I estimate at 8·4, P = 206°·37, D = 282″·4. Epoch 1864·40. In comparing these results with those of other observers, it must be remembered that *β Leonis* has a decided Proper Motion.

Woodcroft Observatory, Cuckfield,
June 10th, 1864.

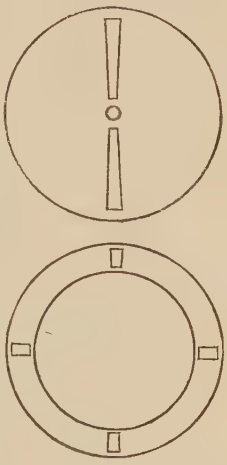
Since writing the above, I have, from the places given in the Palermo Catalogue, deduced the angles of position and distances relative to *β Leonis*, of Piazzi's Nos. 160 and 162, Hora XI., with the following results:—160 P XI., 6·7 mag., P = 202°·5, D = 19′·1; 162 P 11., 8 mag., P = 209°·6, D = 301″·10. Epoch 1800·0.

It may, perhaps, save the risk of confusion if I remark that, in addition to the small star mentioned in the previous communication as agreeing fairly in place with the B of the *Cycle*, there is in the same quadrant a brighter star, 10·6 mag., *n* = 116°·16, D = 556″·5, which has a small comes to the P. *f.*, the co-ordinates of which are P = 21°·8, D = 50″·6.

Woodcroft Observatory, Cuckfield,
June 15th, 1864.

In a letter, dated August 8, addressed to the President, Sir John Herschel proposes an eye-piece for viewing the Sun should be constructed, in which the eye would be defended by

a revolving metal plate, having in it one or two very narrow slits — say two, diametrically opposite, each occupying a sector of the disk — say half a degree. The diaphragm being made to revolve — say five times in a second — a glimpse would be caught at intervals of $\frac{1}{10}$ of a second, which suffices for continuous vision. But the *quantity* of light which would enter the eye would be only $\frac{1}{360}$th part of the total light of the Sun. If the disk were large — say an annulus of 10 inches diameter—and the slits, four in number, were each $\frac{1}{100}$th of an inch in breadth, the annulus revolving once in $\frac{2}{5}$ of 1 second, the quantity of light would be reduced to $\frac{1}{785}$; and if the surface of the annulus next the Sun were of polished metal or looking-glass, the annulus itself would not become very hot.



The annulus and the apparatus setting it in rotation might, of course, be disconnected from the telescope so as not to communicate tremors; for example, fixed to the observer's stage, but, if connected to the tube, a mode of attachment might be devised *cutting off tremors* by the different elasticities of the connecting materials, upon a principle which he devised some years ago, and which has been applied with perfect success (in one of its forms) by the Astronomer Royal to destroy tremors in his mercurial reflector for the Great Circle.

M. Schiaparelli, in a paper entitled "Théorèmes sur le mouvement de plusieurs corps qui s'attirent mutuellement dans l'espace," *Ast. Nach.* No. 1487, establishes, by a simple analysis, the following theorem:—

"When three bodies move in space under the influence of their mutual attractions (according to the inverse square of the distance), the three osculating planes of the trajectories and the plane of the three bodies meet in a point.

"This point is the centre of gravity of the three bodies, each mass being augmented proportionally to the cube of the distance of the other two bodies."

He remarks that the theorem properly modified extends to a system of n bodies attracting each other according to any law; the theorem being, in fact, obtainable from the consideration that, imagining the bodies to become rigidly connected,

the solid body so obtained is in equilibrium under the influence of the mutual attractions. Hence, considering the n resultants of the attractions which act on the n bodies respectively, these results must be forces in equilibrium as regards the aforesaid solid body, and must therefore satisfy the conditions which exist for a system of forces in equilibrium.

In the case of three bodies, the resultants must be in a plane and meet in a point. In the case of four bodies, they must (in accordance with a theorem of Möbius) be generating lines (of the same kind) of a hyperboloid. Whence also the osculating plane of the trajectory of each body cuts the resultants on the three other bodies in three points which lie in a line.

In the case of five bodies, the resultants act along five lines such that there exist two lines cutting all the five lines.

And, in the case of six bodies, the resultants act along six lines connected by the relation which Prof. Sylvester (*Comptes Rendus*, t. lii. p. 741) calls *involution in space*.

The author concludes by remarking that the resultants, whatever be their number, satisfy the six general equations of equilibrium of a solid body. Of these equations, three express that the sum of the components of the forces parallel to three axes is equal to zero. The first three equations lead to the theorem of the conservation of the motion of the centre of gravity; the last three to the theorem of the conservation of areas.

Comet I. 1864.

Discovered by M. Tempel, at Marseilles, July 4.

The following set of elements, calculated from observations of July 9 and 21, and August 5, by Mr. Graham, of the Cambridge Observatory, are given, *Ast. Nach.* No. 1490:—

$$\left.\begin{aligned} T &= 1864, \text{ Aug. } 15{\cdot}533685 \text{ Greenwich M.T.} \\ \varpi &= 246^\circ\ 14'\ 32''{\cdot}9 \\ \Omega &= 95\ 14\ 45{\cdot}2 \\ i &= 178\ 7\ 53{\cdot}5 \end{aligned}\right\} \text{Mean Equinox, Jan. 16.}$$

$$\text{Log } a = 9{\cdot}958759$$

The middle place is represented thus:—

C − O, in Long. = + 0″·4, in Lat. = − 4″·8.

Comet II. 1864.

Discovered by Prof. Donati and M. Toussaint, at Florence, July 23.

The following set of elements, calculated from observations of July 28, July 31, and August 2, are given by M. Krüger, *Ast. Nach.* No. 1487:—

T	=	1864, Oct. 11·088 Greenwich M.T.	
ϖ	=	159 0 26	Equinox, July 31
Ω	=	31 59 12	
i	=	70 23 0	
Log q	=	9·96400.	Motion Retrograde.

Middle Observation, C − O, in Long. = − 4″, in Lat. = − 3″.

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